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PASTEUR'S PROPHYLACTIC TREATMENT OF RABIES.*

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Your president has kindly invited me to read a short paper before the Clinical Society, summarizing the conclusions reached in the recent investigations of Pasteur's treatment of rabies. I consented to do so only reluctantly, because I desired that my first paper before the Clinical Society should contain the results of more independent observations than I have made in this direction, and because so much has been recently written upon this subject that it has been divested somewhat of its interest. My own position in regard to it, however, has been somewhat misunderstood, and it was the desire to correct this erroneous impression that finally overcame my scruples. I have been wrongly regarded as an enthusiastic supporter of and believer in Pasteur's method for the prevention of rabies.

This I have never been, and although at first I was very favorably impressed with the method, because of the results that Pasteur so positively asserted he had obtained in his

* Read before the New York Clinical Society, May 27, 1887.

experiments on dogs, yet I only pleaded then that justice should be accorded to Pasteur for the magnificent work he had already done, and that sufficient time be granted to him to prove or disprove his positions.

It is my purpose in this paper to only present to you the present status of the question, especially from the practical standpoint, as based upon the most reliable information that we have at command, and I shall abstain as far as possible from expressing any individual opinions.

It is almost unnecessary to refer to the methods of Pasteur's treatment, as the principles underlying them have become very well known during the past eighteen months to all medical men.

We may say in brief, however, that in various communications to the Academy of Sciences in 1883, '84, and '85, Pasteur maintained—

1. That the virus of rabies was present in a concentrated form in the central nervous system, and that rabies could be usually produced by the subcutaneous inoculation, and with the greatest certainty by the subdural inoculation, of animals with portions of the brain or spinal cord obtained from animals dead of this disease.

2. That the virulence of this virus could be increased and the period of incubation shortened by the successive subdural inoculation of rabbits.

3. That by a series of inoculations with a rabic virus attenuated by drying, but of a gradually increasing virulence, he had been able to finally render dogs refractory to inoculations with the most virulent virus, and to protect them from the disease after infection with fresh street rabies.

4. That the method was equally applicable and perfectly safe as regards the human being, and that by its use persons bitten by rabid dogs, if inoculated before the period of

incubation had elapsed, could with certainty be protected from the development of the disease.

Let us examine now more carefully each of these statements from the light of recent investigations. The first is that the virus of rabies exists in a concentrated form in the central nervous system, and that rabies can be produced with great certainty by the inoculation of animals with rabic brain or spinal cord. Duboie, in 1879, declared that the virus of rabies was localized in the medulla, and nearly all the investigations that have been made since Pasteur's communication agree in confirming his observations as to the presence of a specific virus in the brain and spinal cord of animals dead of rabies, and as to the possibility of the production of the disease by inoculations with this material. The disease thus produced, however, does not, as a rule, manifest itself by the ordinary symptoms of street rabies, but rather by the symptoms of a progressive paralysis—the so-called dumb or paralytic rabies; this is especially true of the manifestations of the disease in rabbits. The disease, moreover, is produced with far greater certainty, as is claimed by Pasteur, if the inoculations are made by subdural injections of the virus after trephining than if made by subcutaneous inoculations. Frisch, of Vienna, who was sent by the Austrian Government to study Pasteur's method, and who has made a series of elaborate experiments, confirms these conclusions, as does also in fact Abreu, the representative of the Portuguese Government, and Ernest, of Boston, in a carefully conducted series of experiments.

The question at once arises in this connection as to the effects produced by the subdural injection of the medulla from healthy animals. On this point the writer made a number of experiments on rabbits some time ago. In these experiments an emulsion was made in distilled water of the fresh spinal cord of dogs or rabbits. This was injected

underneath the dura mater after trephining in the manner prescribed by M. Pasteur. The amount used varied between one and two c. c. The rabbits quickly recovered from the effects of the chloroform and were soon as lively and well as ever, excepting in two or three instances. In one of these cases the rabbit died in the course of a few hours, and in two others they were somewhat sluggish on the following day, but on the second were quite well and remained so. Aside from these cases, there were no symptoms of any kind following the inoculation, and at the end of several months the animals remained quite well. The results obtained by most other observers have been the same. Spitzka's experiments can not be considered in this connection, as he used for the most part different forms of septic matter and brought about a meningitis, cerebritis, or some other gross lesion which would quite account for the symptoms produced. No such changes are found in animals dead of a rabies produced by subdural inoculation.

The second statement is that, within certain limits, the virulence of the virus is increased and the period of incubation shortened by the successive subdural inoculation of rabbits. So far as I know, no extensive experiments have been made in regard to this point. Von Frisch's experiments, so far as they go, confirm this statement.

The third proposition announces the possibility of rendering animals refractory to inoculations with the most virulent virus by a series of inoculations with an attenuated virus, and the possibility of preventing the development of rabies in animals infected with the virus of ordinary street rabies.

As to the first question, the most carefully conducted and most reliable experiments seem for the most part to confirm this statement. Von Frisch, of Vienna, after a long series of experiments, summarizes his conclusions on this point in

the following words: "Animals that have been subcutaneously inoculated with an ascending series of the spinal cords (attenuated by drying) are protected by the weaker virus against the effects of the stronger if the previous successive inoculations have not followed each other too closely." This last clause contains a very important exception with regard to the later methods of Pasteur, as we shall see later.

Ernest, of Boston, draws the following conclusions as the results of a series of carefully conducted experiments upon this point: "Injections of a virus modified in strength by drying, and in the manner prescribed by Pasteur, exert a very marked protective influence against an inoculation with virus of full strength."

In his experiments, out of nine rabbits which were first inoculated with a strong virus, and then protected by subcutaneous inoculations with an attenuated virus, one only died. In another series, when the strong virus was used and the animals were left unprotected, seven died.

The results obtained by Abreu do not agree with those obtained by other observers. He was sent by the Portuguese Government to Paris to study the subject of rabies, and, after eight months' work in Paris and Lisbon, he made a very full and voluminous report to the Minister of the Interior of Portugal. He says that the rabies produced in rabbits by the inoculations of the medulla of rabid animals appeared after a period varying from one day to five months, but that he was able to produce, in some cases, the same symptoms by the inoculation of rabbits after trephining with the medulla of normal rabbits.

Two Italian investigators, De Renzi and Amoroso, working in Naples, have also reported adverse results.

No conclusive experiments have been made, so far as I know, as to the possibility of preventing the development

of the disease in dogs after infection, in any way, with ordinary street rabies. The experiments of von Frisch are rather adverse to this proposition.

Although the observations at hand are somewhat contradictory, we may at least conclude that the most careful investigations show that the serial inoculations of the weaker virus grant a certain degree of immunity to the action of the strongest virus.

This brings us to the consideration of the fourth point —namely, that this prophylactic method is equally applicable to the human being, and that persons bitten by rabid dogs can with certainty be protected from hydrophobia if they are subjected to the treatment within fifteen or twenty days, or before the period of incubation has elapsed. This is the practical question in the method, and can only be judged of from the results obtained by such inoculations.

The first point that impresses one on carefully reviewing the subject is the enormous number of cases of rabies that have suddenly appeared. Until recently certainly rabies and hydrophobia have been considered very rare diseases in all countries, but in one year after the announcement of this discovery nearly two thousand Frenchmen were subjected to this treatment because they were supposed to have been bitten by rabid dogs, and were in imminent danger of hydrophobia. This astonishing fact at once attracts and holds our attention. If these patients have in reality been bitten by rabid dogs, the disease can no longer be considered a rare one, but must have become a very common one. The most careful and reliable estimates—those of Leblanc—as to the proportion of persons bitten by rabid dogs who develop hydrophobia, place the ratio as at 16 to 100. Bouley, however, says that the proportion fixed by Hunter—*i. e.*, 5 to 100—approaches nearer the truth. If we accept the ratio of 16 to 100, as Pasteur

has done, there would have been 320 deaths from hydrophobia in France in 1886. This is what Pasteur alleges would have occurred. Let us glance now at the statistics of this disease for the last few years in France and other European countries, and see how they compare with such an estimate of the death-rate in France for last year. Boudin, in a communication to the French Academy in 1863, gave the average death-rate from hydrophobia in Prussia, from 1854–'58, as 19·5; in Bavaria, from 1855–'56, as 3·5; in Belgium, from 1856–'60, as 2·6; in England, from 1853–'57, as 10; in Scotland, from 1855–'63, as 1; in Sweden, from 1856–'60, as 4·2.

In Austria the deaths have averaged 8 yearly for the six years ending in 1885. In Prussia the death-rate during the last five years has been 10, 6, 4, 1, and 0, respectively, or 4·2 annually. In Holland the average was 2·1 for the ten years ending in 1879, and during the last five years on deaths have been recorded in the official government reports. In France, according to the reports of Tardieu and Brouardel, for the years 1850–'72, there were in all 685 deaths, or an average of a little less than 30 yearly. Tardieu says of his reports that nearly all the departments replied to his inquiries, and Brouardel that about two thirds were included in his. If we assume that the deaths in the other third were proportionate, we would have a total of 45 annually. It seems safe to assume, however, that the errors in diagnosis in ascribing to hydrophobia deaths which were caused by other affections would more than counterbalance the cases not included in the reports, and there is little danger of any genuine cases of this escaping notice. Now there have been during the year ending Feburary 1, 1887, 14 deaths reported in France from hydrophobia among uninoculated persons, and 25 of Pasteur's patients have succumbed after treatment, making a total of 39. Three of

Pasteur's cases he maintains died of some other disease. If we now allow for unreported cases, as we have done in the other statistics, we have a total greater than 45, which represents the highest reliable previous estimate of the average annual death-rate in France.*

The success that has attended the institutes established in other countries for the prophylaxis of rabies has been, as a whole, less satisfactory than that obtained in Paris. At Moscow 2 deaths have occurred among 115 persons inoculated; at Odessa, 7 deaths in 107 patients.

At Warsaw, if the reports are to be credited, a very sad event has occurred which would in itself almost condemn the whole method if shown to be true: In July a boy was bitten at Lubine by a dog supposed to be rabid, and was immediately subjected to the prophylactic treatment at Warsaw. Contrary to the usual stupid procedure, the dog was *not* killed but was placed in confinement. On the 11th of November the boy died of hydrophobia. At this time the supposed rabid dog was living and well and had shown no further evidences of rabies. The conclusion seems to be inevitable in this case that the boy died from the result of the inoculations.

In St. Petersburg 84 persons have been treated, but no deaths have occurred.

I have appended the table given by M. Lutaud in the "British Medical Journal" for April 2d of the fatal cases treated by M. Pasteur. It agrees entirely with Pasteur's report, with the exception that he maintains that Nos. 9, 12, and 21 in the table died from some other disease.

It seems to me to be a significant fact that 24 of these 25 deaths have occurred during the seven months ending February 1st, only one occurring in the first five months of

* These statistics have been taken for the most part from M. Lutaud's work entitled "La rage."

the year. At this rate we shall have a mortality among Pasteur's French patients of more than 40 annually, which is probably greater than the previous average death-rate. This number does not include the deaths occurring among uninoculated persons.

In October of 1886, because of the large number of deaths following the previous methods of treatment, Pasteur commenced the employment of a new intensive method, varied according to the seat and severity of the bite and the period that had elapsed after the injury. The treatment was varied for three classes of cases as follows:

I. Treatment for small bites through the clothing:

1st day. Three inoculations with medullas 12, 11, and 10 days old.

2d day. Three inoculations with medullas 9, 7, and 8 days old.

3d day. Three inoculations with medullas 6, 5, and 4 days old.

4th day. One inoculation with medulla 3 days old.

5th day. One inoculation with medulla 2 days old.

6th day. One inoculation with medulla 1 day old.

7th day. One inoculation with medulla 4 days old.

8th day. One inoculation with medulla 3 days old.

9th day. One inoculation with medulla 2 days old.

10th day. One inoculation with medulla 1 day old.

II. Treatment for wounds of uncovered parts other than the face. Treatment as above. Several days intervene, and then the series 4, 3, 2, 1 is repeated.

III. Intensive treatment, applied to persons bitten on the head, face, or neighboring parts (neck, etc.), or when the patients have arrived late.

Treatment as before, but the series 4, 3, 2, 1 is repeated a number of times with intervals of two to four days for four, five, or even six weeks.

Among the persons inoculated by this intensive method there have been nine deaths from a form of rabies hitherto almost unknown in the human being—namely, paralytic rabies—a disease similar to that which is produced in the laboratory in rabbits.

The following observations, made by Professor Germe, of Arras, and communicated to the Academy of Medicine of Paris by Professor Peter as quoted by M. Lutaud, can be considered as typical for the nine cases of paralytic hydrophobia noted in December and January :

"L. N., aged forty-two, a basket-maker, was traveling in the country with a little cart under which a dog was tied on November 12, 1886, near Avesmes le Comte; he let his dog loose and was immediately bitten on the right leg. He seized the dog, tied him up, and killed him. The dog had never ceased taking food. The post-mortem examination of the dog was made at Arras by a veterinary surgeon who stated that he could find no symptoms proving that the dog was rabid.

"The corpse was sent to M. Pasteur, and the family is still waiting to know if the dog was rabid or not. N. remained eleven days at M. Pasteur's laboratory, during which he received twenty-one inoculations to the number of even three in one day. After each inoculation he complained of giddiness, felt sick, and vomited.

"On his return to Arras on November 29th nothing remarkable was observed except increase of appetite, which had also been the case while staying in Paris.

"During the nights of December 10th and 11th intense pain was felt in the parts of the body pricked by the inoculations, the pains rapidly invading the spine and continuing until the last moment.

"The patient was restless, and said that he was suffering as he did after the inoculations in Paris, and was sure he would die. A physician who was called in supposed at first that N. was suffering from lumbago, and, later on, from inflammation of the spine.

"The above-mentioned symptoms were followed by great difficulty in breathing, salivation, convulsions of the muscles of the face, arms, and thorax; the patient had nightmares, was restless, and sweated profusely, but there were no general convulsions or hydrophobia. He could swallow easily, except on the last two days of his life. On the 14th two physicians were called in consultation, and they were in doubt as to whether it was an inflammation of the spine or the result of the inoculations. Paralysis set in soon after; the sight got weaker and weaker till it was completely lost; breathing became more and more difficult, and saliva issued abundantly from the sides of the mouth.

"At last the patient died on December 17th at 11 P. M."

As regards this paralytic rabies, it is certain that if it has ever occurred in human beings, it is only in rare instances. The frequency of its development in those patients who have been subjected to the intensive method of treatment certainly has great significance when taken in connection with its symptoms and with similar inoculation experiments on animals.

The conclusions arrived at by von Frisch on this point are interesting.

To the unsuccessful results obtained by him in his first series of experiments with subdural inoculations with strong virus, either before or after the prophylactic treatment, Pasteur has objected—

("Comptes rendus," November 2, 1886) that the preventive inoculations followed each other too slowly, although Dr. v. Frisch had carefully adhered to the procedures described by himself. M. Pasteur then required the application of the whole amount of (preventive) virus within twenty-four hours (the inoculations being made every two hours), and two or three repetitions of the whole series; further, that these inoculations should begin soon after infection, at least on the following day. Experiments thus carried out on dogs and rabbits gave not one

favorable result; all the animals died, even after this "intensive" treatment.

But, further, the important result arises from these experiments that, by a quick succession of inoculations, gradually increasing in virulence, a protective power of the weaker against the subsequent stronger ones is no longer to be safely expected. Of a number of rabbits and dogs which served in control-experiments to the above, and in which the intensive treatment alone was adopted without previous infection, a very large majority died.

Animals that were subjected to preventive inoculations after subcutaneous infection, nevertheless succumbed to rabies, with few exceptions, even when the incubation-period extended to thirty-four days.

At the meeting of the Academy of Sciences on January 11th and 18th the distinguished clinician, M. Peter, after reporting several cases of paralytic rabies, came to the following conclusions—namely, that, first, the death-rate from hydrophobia in 1886, in France, had not been lowered by Pasteur's method; and, second, that the death-rate tended to rise on account of the intensive method. M. Peter then bitterly attacked Pasteur's method, and M. Vulpian for his support of it.

In endeavoring to reach a conclusion as to the efficacy of this method from the experimental results obtained by independent observers and the government commissions appointed to investigate this subject, we are impressed by the scarcity of reliable experimental data. There have been but few experimental researches thus far reported. Those of von Frisch are the most important ones. His conclusions, which have just been given in part, are certainly far from confirmatory of Pasteur's allegations. Abreu, the representative of the Portuguese Government, has written on the fly-leaf of the copy that I have of his report to the Minister of the Interior of Portugal as follows: "I can not

accept the conclusions of the illustrious sage, and it appears to me that his immortal attempt threatens to transform hydrophobia into a common distemper of the human kind, because the public forget the necessity for cleanliness and cauterization of suspicious wounds." The English Government commission are still holding their report for further investigations. The Belgian Government commission reported adversely to the establishment of an institute for these inoculations in Belgium. Two Italian experimenters, De Renzi and Amoroso, have reported adversely. The most distinguished German experimenters, and many of the English, have from the beginning regarded the treatment with suspicion and distrust. On the other hand, some French investigators have confirmed Pasteur's results, and the experiments of Ernest, so far as they go, are in quite close accord with the conclusions of Pasteur. The conclusions that he draws from his experiments are:

I. There exists in the cords and brains of animals inoculated in Pasteur's laboratory a specific virus capable of the production of similar symptoms through a long series of animals.

II. That these symptoms are produced with absolute certainty when the method of inoculation is by trephining the skull and injection under the dura mater; with less certainty when the inoculation is by subcutaneous injection.

III. That the strength of this virus is lessened when the cords containing it are removed from the animals and placed in a dry atmosphere at an even temperature.

IV. That the symptoms produced by the inoculation of this virus only appear after a certain period of incubation distinctly shorter when the inoculation has been done by trephining than when done by subcutaneous injection.

V. That injections of the virus modified in strength by

drying, and in the manner prescribed by Pasteur, exert a very marked protective influence against an inoculation with virus of full strength.

VI. That a very moderate degree of heat destroys the power of the virus entirely, while prolonged freezing does not injure it.

It has been found, however, that rabies can not be produced with certainty by the subcutaneous inoculation of the virulent virus. In the majority of cases the disease may be produced, but this result is not invariable, and in the experiments from which these observations were drawn the virus was introduced in this manner. The disease, however, can be produced with the greatest certainty by inoculations underneath the dura after trephining. The question immediately arises, What effects are produced by subdural inoculations with virulent virus either before or after the prophylactic treatment? In Ernest's cases, 50 per cent. died when strong virus was used this way, either before or after the prophylactic treatment. That 50 per cent. lived would seem to show that there certainly was a marked prophylactic action exerted by the treatment, for, when not protected by treatment, all the animals die after subdural inoculation with strong virus. Von Frisch, however, has not obtained such good results, and I append his conclusions upon this point:

Animals that have been subcutaneously inoculated during the course of ten days with virus material (that is, portions of spinal cord dried for different periods) of gradually increasing virulency, contrary to M. Pasteur's statements, do not possess perfect immunity against infection with fresh "street rabies," and, after subdural infection with the latter, remain healthy only in exceptional cases.

Rabbits and dogs that were subjected to preventive inoculations after trephining and subdural infection with "street rabies" (of sixteen days' incubation period), all died of rabies

with one single exception. (See "Anzeiger der k. k. Academie der Wissenschaften," July 15, 1886; also "Medicinische Presse," 1886, No. 32.) The animal, moreover, that remained healthy was infected subdurally fourteen weeks later, and died of rabies eight days afterward.

Among distinguished members of the French medical profession, Pasteur's most bitter opponents are M. Peter and M. Lutaud, the able editor of "The Journal of Medicine of Paris." M. Lutaud, in a careful and comprehensive review of the whole subject, in a work entitled "La rage," charges Pasteur with a perversion of statistics, of want of accuracy in his work, and of unworthy motives. He concludes, with M. Peter, first, that the mortality from hydrophobia in France has not been decreased by his inoculations; and, second, that it tends to rise as the result of his recent intensive method of treatment.

It is quite impossible, in this short paper, to take up the consideration of the many disputed questions or the experimental evidence brought forward by Pasteur to show that the majority of the 2,000 Frenchmen inoculated were bitten by rabid dogs. The experimental evidence at hand from other sources is contradictory and insufficient. Von Frisch's experiments are lacking in the convincing proof of his conclusions. Ernest's are more carefully detailed, but do not cover all of the most important ground. We find, moreover, a considerable discrepancy in some of his results as compared with Pasteur's statements. Ernest has thrown out of his table of thirty-two rabbits inoculated by trephining five animals. These showed no symptoms, or only slight ones, and were excluded on the ground that the virus was attenuated or destroyed by heating. To prove this statement, he has reported one additional experiment, in which the virus was heated for five minutes to 110° F., and in which no symptoms followed

the inoculation. This experiment, in itself, evidently proves nothing.

On the other hand, M. Gibier, in a note to the Academy of Sciences, dated June 11, 1883, stated that the virus of rabies was attenuated by the influence of a temperature varying between 40° to 46° C., or 104° to 115° F. Gibier's experiments were repeated by M. Pasteur, and in a later communication Gibier's conclusions were stated by him to be erroneous. (See Vignal's "Report," "Brit. Med. Journal," April 17 and 24, 1886.) This, of course, is not a matter of paramount importance, but involves the modification of Ernest's second conclusion, and the rejection of a part of the sixth.

Dr. Valentine Mott has told me that in his inoculations death has almost invariably followed after subdural injections of the virus. In the few cases where this result did not follow, he had thought the virus had not passed under the dura, or was too small in quantity. When these animals were inoculated a second time, death always followed. Death had never occurred later than the fourteenth day in his cases, and very rarely had reached this limit. In Ernest's cases 14·9 days was the average time, and twenty-eight days the longest period.

As regards the experimental data at hand, we may conclude that they are contradictory, insufficient, and incomplete. So far as the results obtained go, they show that there is some peculiar nerve poison that Pasteur is experimenting with. This poison is probably that of rabies, has a specific action on certain animals, may be attenuated by keeping in a dry air for various periods of time, and when thus attenuated and used for inoculation in a prescribed manner, grants a certain degree of insusceptibility to the strongest virus. The insusceptibility thus conferred, however, is not absolute.

We will return now to the practical results obtained in the inoculation of human beings. It seems quite incredible that nearly 2,000 French persons have been bitten by rabid dogs in 1886. It is fully as incredible that there would have been 320 deaths from hydrophobia in France in 1886 if Pasteur's inoculations had not been made. The estimated average mortality in France from 30 to 45 annually for many years, as shown by the most reliable statistics at hand when compared with deaths in other countries from this disease, and with our general knowledge of the rarity of the disease, seems a very liberal estimate, and I should say is certainly not too low. The deaths during the last year have been, roughly speaking, somewhere between 35 and 40. *Now, if these statistics are correct*, we are forced to the conclusion that there is as yet no proof from this side of the question of the efficacy of the method. Again, M. Pasteur has placed the number of deaths in the Paris hospitals during the last five years at 60, or 12 annually. This year he says there have been only three—one of his patients, and two who had not been inoculated. Lutaud, on the other hand, gives in detail a statement of each death from hydrophobia in each of the Paris hospitals for the last five years, and finds that the total number is 26, or 5·1 annually, and the total number for the last eleven years as 43, or 3·9 annually, instead of 12, as stated by Pasteur. Further, he says that two of Pasteur's patients have died in the Paris hospitals in 1886, making the total number of deaths 4 instead of 3, as Pasteur has stated. These statistics again if correct, show nothing favorable to Pasteur.

Now as to the new method of intensive treatment, which appears to have been subjected to innumerable modifications. This, it seems to me, has no rational foundation as viewed from any standpoint. It does not harmonize with anything that we know concerning the virus of any

of the infectious diseases. It is as unscientific in its conception as it is irrational in its application, and is opposed to Pasteur's own previous statements and belief. Further, so far as we know, it had no sufficient test in experiments on animals before it was applied to man, and the only independent experiments thus far made with it wholly condemn it. The method of development of paralytic rabies in the human being, the history and manifestations of the disease, the experimental results obtained in animals, and the case of hydrophobia at Warsaw, all seem to make it at least possible that the disease may be actually produced by the inoculations.

After this hasty review of the whole subject it does not seem to me that the conclusion drawn by von Frisch from his experiments is without foundation when he says: "That M. Pasteur's method of conferring immunity on animals against the virus of rabies requires much further working out before it can be considered safe or trustworthy, while as to human beings there are as yet no valid reasons for the institution of a preventive treatment. On the contrary, there is a strong presumption that the actual disease may be produced by the preventive treatment itself, at least in the intensive form lately adopted by Pasteur."

In a paper read nearly one year and a half ago the writer said: "It seems to me that the strongest evidence of the efficacy of the method for the prevention of rabies rests, not upon any results thus far obtained in the inoculation of human beings, but upon the results of his experiments upon dogs. But these experiments are as yet unconfirmed by other observers, and scientific men will hesitate to accept such far-reaching conclusions as are involved in this method without such confirmation." And quoting again from that paper: "Pasteur's prophylactic method for rabies

rests purely on empirical grounds, and can only be fairly judged by the practical results obtained by its use." After the lapse of a year and a half we can only say that the experiments upon animals made by other observers have been only in part confirmatory of his position, and that the practical results thus far obtained lack very much of being what Pasteur declared they would be. With scarcely any more confirmatory evidence than we had at that time, and with much opposing evidence derived from the practical results obtained by the application of the method, we may well follow in the footsteps of the English hydrophobia commission and wait for fuller and more reliable information.

TABLE GIVING THE MORTALITY AMONG THE FRENCH PATIENTS TREATED IN M. PASTEUR'S LABORATORY IN 1886.

No.	Name.	Age.	Part of body bitten.	Date of bites.	Dates of treatment.	Description of treatment.	Date of death.	Cauterized or not.	Animal.	Observations.
1	Videau, Matthieu.	3	Forehead.	Feb. 24.	Feb. 27 to March 7.	Spinal cords of 14 to 5 days.	Sept. 24, 1886.	Not.	Dog.	Slightly bitten; incubation remarkably long; convulsive hydrophobia; incubation, 200 days.
2	Lagut, Elvina.	11	Inferior lip.	May 18.	May 24 to June 2.	Spinal cord of 14 to 5 days.	June 17, 1886.	"	"	Convulsive hydrophobia, canine.
3	Bouvier, Marius.	40	Hand.	April.	....	.....	July 21, 1886.	"	Cat.	Convulsive hydrophobia; M. Pasteur's supporters say that it was a case of deliri'm tremens; incubation long.
4	Cledière, Emile.	21 mo.	"	June 17.	June 21 to June 30.	.....	Aug. 17, 1886.	"	Dog.	
5	Peytel, Henri	6	"	June 28.	June 30 to July 9.	.....	July 16, 1886.	"	"	Canine hydrophobia; incubation, 18 days.
6	Leduc, Zélie.	70	"	July 14.	July 18 to 25.	.....	Sept. 10, 1886.	"	"	Convulsive hydrophobia; incubation, 50 days.
7	Magneron, Norbert.	18	"	July 25.	August 1 to 7.	.....	Oct. 16, 1886.	Cauterized 3 days after.	"	Convulsive hydrophobia; incubation, 81 days.
8	Moerman, Alfred.	40	"	June 28.	August 11 to 21.	.....	Sept. 7, 1886.	.....	"	Convulsive hydrophobia.
9	Christin.	12	Forehead.	June.	July 1 to 10.	.....	July 17, 1886.	Not.	"	The laboratory says the child died from meningitis, but the post-mortem examination, made at the hospital at Evian, goes against that opinion; the child died of hydrophobia after having been bitten.
10	Moulis, André.	6	Forearm.	July 31.	August 6 to 12.	Spinal cords of 14 to 5 days.	Sept. 8, 1886.	Cauterized.	"	
11	Grand, Louis.	41	Hand.	Sept. 5.	Sept. 14 to 28.	.....	Sept. 8, 1886.	"	"	
12	Duresset, Edouard.	..	Leg.	August.	September.	.....	End of Sept.	?	"	The death, which happened one month after the inoculations, is said to have been due to pneumonia; the patient was treated by Dr. Yot, at Versailles.
13	Astier, Justin	2	Both cheeks	Aug. 4.	August 5 to 21.	.....	Sept. 16, 1886.	Cauterized.	"	
14	Jansen, Louis.	47	Legs and fists.	Aug. 18.	Aug. 21 to Sept. 3.	.....	Dec. 31, 1886.	Not.	"	Convulsive hydrophobia; incubation, 146 days.
15	Clergot, Eugène.	27	Forearm.	Aug. 7.	August 11 to 23.	.....	Oct. 24, 1886.	"	"	
16	Lodini, Bernard.	46	Leg.	Oct. 12.	October 21 to 31.	New treatment, described on Nov. 2, 1886.	Nov. 24, 1886.	"	"	Paralytic hydrophobia; pain at the points injected.
17	Leteng, Etienne.	59	Foot bitten through slipper.	Nov. 3.	November 8 to 20.	.....	Dec. 8, 1886.	"	"	Paralytic hydrophobia; pain at points injected, not at part bitten.
18	Née, Leopold	42	Leg bitten through trousers.	Nov. 21.	November 17 to 26.	.....	Dec. 17, 1886.	"	"	Ditto.
19	Gerard, Amédée.	28	Hand.	Dec. 1.	December 3 to 13.	.....	Jan. 3, 1887.	"	"	Ditto.
20	Reveillac, Louis.	25	"	.....	.....	.....	.....	"	"	Ditto.
21	Rouyer, Arthur.	12	"	Oct. 20.	Oct. 25 to Nov. 5.	.....	Nov. 26, 1886.	"	"	Ditto. (According to M. Brouardel, a case of uræmia.)
22	Goriot, Paul.	12	Forefinger.	Sept.	End of December.	.....	Jan. 14, 1887.	"	Cat.	Ditto.
23	Foulup.	30	Hand.	Dec. 1.	December 12 to 22.	.....	Jan. 24, 1887.	"	Dog.	Ditto.
24	Alphand.	42	"	Dec. 13.	Dec. 20 to Jan. 1.	.....	Jan. 20, 1887.	"	"	Ditto.
25	Bergé.	40	"	Sept.	Sept. 12 to 24.	.....	Jan. 28, 1887.	"	"	Ditto.

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